

## LETTERS TO THE EDITOR.

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## Radium Emanation.

IN 1903, it was shown by Mr. Soddy and myself that the spontaneous change of the emanation from radium results in the formation of helium; this observation has been confirmed by Indrikson, by Debierne, by Giesel, by Curie and Dewar, and by Hinstedt and G. Meyer. Debierne has shown that actinium chloride and fluoride also develop helium. I have also once detected helium in the gases evolved continuously from a solution of thorium nitrate, and hope soon to confirm this observation.

When the emanation is in contact with, and dissolved in water, the inert gas which is produced by its change consists mainly of neon; only a trace of helium could be detected.

When a saturated solution of copper sulphate is substituted for water, no helium is produced; the main product is argon, possibly containing a trace of neon, for some of the stronger of its lines appeared to be present. The residue, after removal of the copper from this solution, showed the spectra of sodium and of calcium; the red lithium line was also observed, but was very faint. This last observation has been made four times, in two cases with copper sulphate, and in two with copper nitrate; all possible precautions were taken; and similar residues from lead nitrate and from water gave no indication of the presence of lithium; nor was lithium detected in a solution of copper nitrate, similarly treated in every respect except in its not having been in contact with emanation.

These remarkable results appear to indicate the following line of thought:—From its inactivity it is probable that radium emanation belongs to the helium series of elements. During its spontaneous change, it parts with a relatively enormous amount of energy. The direction in which that energy is expended may be modified by circumstances. If the emanation is alone, or in contact with hydrogen and oxygen gases, a portion is "decomposed" or "disintegrated" by the energy given off by the rest. The gaseous substance produced is in this case helium. If, however, the distribution of the energy is modified by the presence of water, that portion of the emanation which is "decomposed" yields neon; if in presence of copper sulphate, argon. Similarly the copper, acted upon by the emanation, is "degraded" to the first member of its group, namely, lithium; it is impossible to prove that sodium or potassium are formed, seeing that they are constituents of the glass vessel in which the solution is contained; but from analogy with the "decomposition-products" of the emanation, they may also be products of the "degradation" of copper.

A full account of this research will shortly be communicated to the Chemical Society.

WILLIAM RAMSAY.

July 11.

## Effect of Pressure on the Radiation from Radium.

I HAVE, during the last eighteen months, been engaged in an investigation on the effects of pressure on radio-active phenomena. In designing the apparatus necessary for the purpose, it was necessary to consider that if any change in the rate of production of the emanation occurs through pressure, effects would not be noticeable at once, as a new state of equilibrium would only be reached after several days. Similar considerations hold if any of the slowly decaying products is affected. A special pressure pump was therefore constructed according to the designs of Mr. J. E. Petavel, and this pump allowed me to keep up a pressure of about 2000 atmospheres almost indefinitely without sensible leak. The time of the experiments was not, however, extended beyond four or five days. The results have been entirely negative, and I estimate that a change in the activity of one-third per cent. would have been noticed.

During the course of the investigation several fictitious

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effects made their appearance, and it was the elimination of these which necessitated a gradual improvement in the methods of observation and took up the greater part of the time occupied in the experimental inquiry.

In addition to the help of Mr. Petavel which has already been mentioned, I have had the assistance of Mr. Makower in the early stages of the work. The final experiments were conducted by my assistant, Dr. Hans Geiger.

ARTHUR SCHUSTER.

Victoria Park, Manchester, July 12.

IN order to ascertain if the rates of disintegration of radium and its successive products (the emanation, A, B, C) are affected by high pressure, we have placed about 1 gram of barium chloride, containing 1.04 mg. of radium, completely sealed beneath lead, in a thick-walled cylinder of nickel steel, and compressed the radium by a tight-fitting chromium tungsten steel piston 1 cm. in diameter. The greatest pressure applied has been  $3.2 \times 10^5$  lb. to the square inch, which is the estimated pressure at a depth of fifty miles beneath the surface of the earth. The penetrating radiation arising from radium C was observed by two large electrosopes placed on either side of the radium, and at a distance of about 30 cm. from it. The  $\gamma$  rays produced a deflection of about twenty-eight divisions a minute in an electroscope, the natural leak of which was 0.4. The pressure on the radium was gradually increased from zero to that at ten, twenty, thirty, forty miles beneath the earth's surface, and was maintained for four days at about the forty-mile value. The pressure was then taken off, and observations were continued for three days more. During all these variations of pressure, no change was detected in the  $\gamma$  radiation, although a variation of 1 per cent. could have been observed without difficulty.

The pressure was then rapidly carried from zero to the fifty-mile value and back, and also maintained at fifty miles for two hours. Again there was no change, certainly not 1 per cent.

It is therefore clear that the transformation from radium to radium C continues in a normal manner at pressures equal to those at forty to fifty miles beneath the earth's surface; and this important conclusion seems inevitably to follow—that radium generates heat by disintegration equally at the surface of the earth and at pressures which obtain at depths forty to fifty miles beneath the surface.

The Hon. R. J. Strutt has proved that the quantity of radium in rocks near the earth's surface is greatly in excess of that required to compensate for the loss of heat by conduction and radiation from the earth's surface. Dr. Bronson has proved that the disintegration of radium is unchanged by wide variation of temperature. It appears from our experiments that the transformations take place in the usual manner even under a pressure of 160 tons to the square inch. If radium were distributed throughout the earth in the same amount as at the surface, a higher temperature gradient than that actually found would be expected. A possible explanation of the paradox has been put forward by Strutt and supported by Milne. He supposes that the constituents of the earth some twenty to forty miles beneath the surface are different in character from those near the earth's surface, and that they do not contain radium, or contain it to a smaller extent. This seems to carry with it the conclusion that igneous rocks, which contain considerable quantities of radium, have their origin nearer the surface of the earth than some geologists have supposed.

A. S. EVE.

FRANK D. ADAMS.

McGill University, Montreal, June 28.

## The Æther and Absolute Motion.

THE particular objection to identifying magnetic force with velocity of the æther, which has been discussed recently in the columns of NATURE by Prof. O. W. Richardson, Sir Oliver Lodge and Prof. W. M. Hicks, Dr. C. V. Burton and Mr. E. Cunningham, must depend on some point of view which is foreign to my ways of thinking. Such a hypothesis involves, of course, that the

all-pervading æther shall be at rest under normal conditions; the effect of any local disturbance due to matter must thus be a local effect, and the distant regions of æther will remain unmoved. There can be no question of ascribing a uniform motion to the whole of the æther, extending to the remotest infinity, because there is no conceivable means of producing or altering such a motion. In other words, an infinitely extended æther postulates absolute motion as a fact, in the only real sense of that term, namely, motion relative to the remote quiescent regions of the æther; and once that determination is made, arguments from relativity of motion must lapse.

The interesting point raised by Prof. Richardson, that the steady field of a uniformly moving electron would contain an infinite amount of moment of momentum, requires detailed consideration; but it is not without parallel in more familiar departments of abstract physics. Its scope may be illustrated by the steady motion of a solid sphere in infinitely extended viscous fluid. The sphere, even when in steady motion, experiences resistance, and must be pushed along in order to maintain its motion. This steady push must impart momentum to the fluid, which increases in amount without limit as the time is prolonged; and it is, in fact, well known that the field of flow around the sphere when it has reached its ultimate theoretical steady state contains an infinite amount of linear momentum. But this circumstance does not vitiate the dynamics of fluid resistance. For, in fact, the steady state of motion is very soon set up throughout the neighbourhood of the moving sphere, while the continued supply of momentum simply diffuses away into the distant regions where the velocity is so slight that it does not react sensibly upon the resistance to the sphere. Similar considerations apply to the case of an electron set into steady translatory motion through æther. Here it is rotational momentum that is steadily imparted to the surrounding æther as time goes on, and is carried away into the distant regions by wave-motion. This requires that the æther exerts a torque on the moving electron, the reaction of which on the æther is the source of the angular momentum communicated to that medium. The possibility of permanent adjustment to a torque of this kind is not here anomalous; it is provided for in the fundamental hypothesis of elastic resistance to absolute rotation of the parts of the æther. There is, however, a fundamental difference from the previous illustration of a solid sphere moving through infinite viscous fluid. In that case the force continually does work, leading to continued dissipation of energy into the viscous fluid. But in the electric case the energy in the æther settles down to a steady value, and no further energy is put into it, although a constant stream of angular momentum is put into it so long as the motion of the electron goes on.

The validity of illustrating the nature of magnetic force by velocity of the æther rests on an application of the Principle of Least Action. The power of that principle resides in its allowing dynamical inferences to be drawn without requiring detailed scrutiny of the mechanism through which the forces operate. But the exceptional character of the hypothesis of rotational elasticity, or the possibility of some flaw in the argument, might conceivably have put the application of the principle at fault. It is thus essential, both for verification and for clearness of view, to scrutinise in detail the circumstances of the motion as determined by the Principle of Action, so far as possible. It would appear that, as regards the interesting feature discussed by Dr. Richardson, nothing has gone amiss.

Even in the case of a sphere set in motion in frictionless fluid, it may be said in the same way that when the steady motion has become absolutely established by propagation outward, an infinite amount of momentum has been transferred from front and rear to the sides.

Cambridge, July 8.

J. LARMOR.

IN NATURE of July 4, Mr. E. Cunningham discusses my statement of an objection to identifying the magnetic vector with translational velocity of the æther. Mr. Cunningham says:—"If it were definitely stated that the magnetic force in the free æther was proportional to the velocity of the

æther relative to the observer the objection would be valid"; and certainly any theory which embodied such a doctrine would stand self-condemned. My argument, however, was not directed against this obviously untenable view.

If, for the free æther, we assume that magnetic force is, within a constant factor, identical with æthereal velocity, then we can determine the velocity of the æther at any point by measuring the magnetic force at that point. For the value of the magnetic force there is a perfectly definite physical criterion, which is independent of any arbitrarily chosen frame of reference, and hence we should have the means of determining *absolutely* the velocity of the æther. Thus, on our assumption, absolute motion attains to a definite physical significance which has no counterpart in the postulates of ordinary dynamics; and, this being so, dynamics must fail to give a true account of electromagnetism.

Accordingly, when we identify the magnetic vector with translational æthereal velocity, and at the same time assume that we are dealing with a dynamical system, we should expect to be led to results inconsistent with known electromagnetic relations. Consider, in fact, the unaccelerated motion of a charged body (or of an electron) through a space where there is no magnetic intensity. Then pass to the case where the space in question is uniformly pervaded by magnetic force in a direction transverse to the motion of the charged body, while at the same time a new component is added to the motion of the body, identical in direction and magnitude with the æther-drift which we assume to constitute our magnetic field. We have thus impressed a uniform velocity on the whole system considered, and if the system is fundamentally a dynamical one, no new acceleration will thus have been introduced: that is, the charged body will move without acceleration across the lines of magnetic force.

C. V. BURTON.

Cambridge, July 12.

#### Root Action and Bacteria.

IN NATURE of June 6 (p. 126), Mr. S. Pickering has a note on "Root Action and Bacteria" in which he concludes that the proper functioning of roots depends on the presence of bacteria.

Experiments I have made here both in the field and in water culture with a large number of varieties of plants do not lead directly to the same conclusion. Water cultures have been made on a fairly large scale, three harvests of ninety plants being taken in a large number of dishes, each containing two litres of water. After the third harvest, the water was allowed to stand and evaporate to half its volume. On attempting to use this water for further water cultures, all the plants sown in it died within two days and some within half an hour, the time varying with the variety of plant that was transplanted into the water, and with the variety that had previously been grown in the water.

Further, boiling this water neither increases nor diminishes its toxicity to plants. It is, moreover, immaterial whether the nutrient solution is such as to become acid or alkaline after use; neutralisation in either case does not make it possible to grow plants in it.

My experiments lead to the conclusion that the roots of all the plants so far tried excrete a substance that is toxic to all plants (including that by which it is excreted), but in different degrees.

Similarly, in the field *Sesamum indicum* will not grow (on stiff black soil) within 2 feet of *Sorghum vulgare*; all the plants tried appear to decrease the yield of neighbouring plants of the same variety by about 50 per cent.

The effect of the toxic substance both in the field and in water culture is completely neutralised by tannic and pyrogallic acids, carbon black, and other substances.

It is, of course, possible that bacteria in the soil have a beneficial action by elaborating antitoxins similar to those mentioned.

I have not yet been able to isolate the toxic substance contained in the polluted water of my cultures.

F. FLETCHER.

Poona, June 21.